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HISTORICAL EXPOSITION OF MECHANICS.

The Science of Mechanics: a Critical and Historical Exposition of its Principles. By Dr. Ernst Mach, Professor of Physics in the University of Prague. Translated from the second German edition by Thomas J. McCormack. (Chicago: The Open Court Publishing Co. London: Watts and Co., 1893.)

THE appearance of a translation into English of this remarkable book should serve to revivify in this country the somewhat stagnating treatment of its subject, and should call up the thoughts which puzzle us when we think of them, and that is not sufficiently often.

Prof. Mach is a striking instance of the combination of great mathematical knowledge with experimental skill, as exemplified not only by the elegant illustrations of mechanical principles which abound in this treatise, but also from his brilliant experiments on the photography of bullets, which have been recently elaborated and simplified by Mr. C. V. Boys.

The appearance of the first edition, in 1883, is stated in the preface of the second edition, to have stimulated the production of treatises by Wohlwill, Streintz, Lang e, Epstein, Müller, Popper, Helm, Planck, Poske, and others, discussing theories of cognition in connection with Mechanics; but it is curious that Maxwell's little tract on "Matter and Motion," of 1879, a veritable master-piece on this subject, should appear to be unknown to the author, although mentioned in the translator's foot-notes.

The present volume is not a treatise upon the application of the principles of mechanics, to quote from the preface. Its aim is to clear up ideas, expose the real significance of the matter, and get rid of metaphysical obscurities. Mechanics is treated not as a branch of mathematics, but as one of the physical sciences; and the reader's interest is invited to know how the principles of mechanics have been ascertained, from what sources they take their origin, and to what extent they may be regarded as permanent acquisitions. All this, the positive and physical essence of mechanics, which makes its chief and high interest for a student of nature, is in existing treatises completely buried and concealed beneath a mass of technical considerations.

The gist and kernel of mechanical ideas has in almost every case grown up in the investigation of very simple and special cases of mechanical processes; and the analysis of the history of the discussions concerning these cases must ever remain the method at once the most effective and the most natural for laying this gist and kernel bare. Indeed, it is not too much to say that it is the only way in which a real comprehension of the general upshot of mechanics is to be attained.

Acting upon the plan laid down in the citations above, the author treats his subject from the historical order of development, and begins with an Introduction of general remarks, illustrated by an Egyptian representation of the mechanical arts, and by a quotation from Vitruvius.

Chapter i., on the Development of the Principles of Statics and Hydrostatics, follows closely the historical subject-matter given by Lagrange in the first section of his "Mécanique analytique"; but Prof. Mach's treatment has the very great superiority of being profusely illustrated by elegant diagrams, while Lagrange made a point of banishing figures entirely from the "Mécanique analytique."

Lagrange thus appears as a supporter of the ancient tradition that Rational and Practical Mechanics were to be considered as in a measure opposed to each other, as Newton observes in his preface of the "Principia"; the latter being an inferior branch of study, to be cultivated only for the sake of gain or some other material advantage.

To quote from Rankine's Preliminary Dissertation, prefixed to his "Applied Mechanics":—

> "Archytas of Tarentum might illustrate the truths of Geometry by mechanical contrivances; his methods were regarded by his pupil Plato as a lowering of the dignity of science. Archimedes, to the character of the first geometer and arithmetician of his day, might add that of the first mechanician and physicist—he might, by his unaided strength acting through suitable machinery, move a loaded ship on dry land—he might contrive and execute deadly engines of war, of which even the Roman soldiers stood in dread—he might, with an art afterwards regarded as fabulous till it was revived by Buffon, burn fleets with the concentrated sunbeams; but that mechanical knowledge and that practical skill, which in our eyes render that great man so illustrious, were by men of learning, his contemporaries and successors, regarded as accomplishments of an inferior order, to which the philosopher, from the height of geometrical abstraction, condescended with a view to the service of the State."

We have only to study the progress of the essentially modern science of Electricity to recognise the eloquent truth of Rankine's words, in inveighing against the mediæval and ancient fallacy that there is a *double system of natural laws*, one theoretical, geometrical, rational, discoverable by contemplation, applicable to celestial, ætherial, indestructible bodies, and being an object of the noble and liberal arts; the other practical, mechanical, empirical, discoverable by experience, applicable to terrestrial, gross, destructible bodies, and being an object of what were once called the vulgar and sordid arts.

Possessed with these prejudices, the scholar of ancient and mediæval times, and even of the present day, was occupied in developing and magnifying the numerous errors, and in perverting and obscuring the much more numerous truths which are to be found in the writings of Aristotle; so that it is not surprising that the notion arose of scientific men being unfit for the business of life, and that various facetious anecdotes were contrived illustrative of this notion, anecdotes which have been handed down from age to age, and applied with little variation to the eminent philosophers of every time.

Returning to chapter i. of Prof. Mach's treatise, we find that the Principle of the Lever, employed in the writings of Archimedes, is the real foundation of the Science of Statics, and not the Parallelogram of Forces, as is generally taught; this theorem, although sketched out by Stevinus of Bruges, was first fully enunciated by Varignon and Newton, about 1687. In fact, the modern treatment of Statics is almost entirely due to Varignon.

Prof. Mach examines with the acumen of a metaphysician the weak points of a demonstration; it is a pity then that he seems unacquainted with Duchayla's proof of the Parallelogram of Forces, which is unfortunately so popular with writers on mechanics in this country, as he would have revelled in pointing out the weakness of a logic which prides itself above all things on its rigour.

The Principle of Virtual Velocities, employed as fundamental by Lagrange in his "Mécanique analytique" in preference to the principle of the Parallelogram of Forces, was enunciated very clearly by Stevinus in its application to systems of pulleys; and here we are compelled to call attention to a flaw in Fig. 39, *c*, the only one that we have met in the course of the work; the system cannot possibly be in equilibrium with the central portion of the thread askew, as drawn in the diagram.

The Principle of Virtual Velocities is important in the historical development of Mechanics as the first sketch and shadowing forth of the modern Principle of the Conservation of Energy; but it is unfortunate that the name should still survive, as it is confusing and meaningless. Prof. Mittag Leffler was eloquent at the meeting of the British Association at Oxford in his denunciation of the habit of attaching to theorems certain names of individuals, real or quasi discoverers; and he might have quoted the Principle of Virtual Velocities as an instance of the disadvantage of inventing a descriptive title of too great generality to a newly discovered theorem.

Lagrange has attempted an experimental verification of the Principle of Virtual Velocities, and it is a tradition that an apparatus was constructed on these indications by a former professor of mathematics at Cambridge. It is probable that the description of this demonstration and of the apparatus to be found in Todhunter's *Analytical Statics* was purposely ironical; and that, in popular language, Todhunter wrote this with his tongue in his cheek, knowing the story of the sceptical student who had tried the experiment himself.

The demonstration amounts to proving that a certain weight is no more likely to rise than to fall, and therefore (here Todhunter says he follows Lagrange's words very closely) the weight should remain stationary. The student, however, found that the weight did not remain stationary, and wanted to know why; the professor told him in confidence that it was prudent to make use of an invisible pin to keep the weight in order.

This is not the only case in which it is desirable for the professor to keep a card up his sleeve, as the saying is; in the Foucault experiment of the pendulum which shows the rotation of the Earth, the slightest current of air will destroy and reverse the desired motion; so that it is advisable in showing the experiment to have an elastic ball concealed in the palm of the hand, which can send a slight current of air on the bob of the pendulum, and thus accelerate the initial precession of the plane of the vibration so as to gratify the eyes of the audience and diminish their impatience at the slowness of the motion; afterwards the motion can be checked so that the total advance is made to agree with the theoretical result. Very undignified and dishonest, some will say; but the experiment is otherwise bound to fail from its delicacy when shown to a large audience, except under the most favourable conditions.

While Statics, both as a Science and an Art, can be traced back through Archimedes and the existing monuments of the Egyptians, Greeks, Romans, and of mediæval architects, the Principles of Dynamics, discussed in chapter ii., were first laid down clearly by Galileo; and the great fallacy to be destroyed before any real advance could be made was that of the Aristotelians, who maintained that heavy bodies fall faster than light ones, because the upper parts weigh down on the under parts and accelerate their descent. But in that case, retorted Galileo, a small body tied to a larger body, must, if it possesses *in se* the property of less rapid descent, retard the larger; *ergo* a larger body falls more slowly than a smaller body.

The entire fundamental assumption is wrong, as Galileo says, because one portion of a falling body cannot by its weight under any circumstances press another portion; although, according to Wohlwill (Appendix I.), even Galileo himself only very gradually abandoned the Aristotelian conceptions.

But discarding all metaphysical argument, the Aristotelian fallacy was demolished once for all by the *experimentum crucis* carried out by Galileo, of letting bodies of different weight fall from the Leaning Tower of Pisa, when all were found to take the same time of descent; any slight discrepancies were afterwards accounted for by the resistance of the air.

A still more delicate experimental verification is to be found in the pendulum, as pointed out by Galileo; a plummet at the end of a thread has the same period of oscillation whatever be the weight or the material of the plummet.

The theory of the pendulum, when composed of a body of finite size, as required in a clock, was completed by Huygens in his *Horologium oscillatorium*, 1673, in which the mutual controlling influence of a number of separate plummets is investigated when the plummets are rigidly attached together; and thus for the first time the idea of a moment of inertia and of a centre of oscillation was introduced into Mechanics. In his further researches into the theory of the clock, Huygens was led to the discovery that isochronism for all amplitudes can be secured by making the plummet oscillate in a cycloid; and to do this practically he found that the thread must wrap and unwrap on an equal cycloid, and thereby he made the first step in the doctrine of evolutes and the theory of the circle of curvature.

Having demolished the Aristotelian fallacies on falling bodies, Galileo had still to determine the true laws; his first conjecture that the velocity grew uniformly with the distance, or that $v = gs$, having proved untenable, Galileo stumbled upon the true law that the velocity grows at a constant rate, or that $v = gt$.

The next step in the theory, to prove that in consequence $s = \frac{1}{2}gt^2$, was not an easy matter for Galileo, who sought in general an experimental proof of his theorems (as, for instance, his attempted quadrature of the cycloid by weighing it made in sheet lead); once this was established, however, it was comparatively an easy matter to demonstrate that the path of an unresisted projectile is a parabola, and to prove it experimentally by rolling a ball obliquely on an inclined table.

Galileo's difficulty was to measure lapse of time with exactitude, accurate clocks and watches not being in existence in his day; but he overcame this difficulty by a modification of his own invention of the ancient Clepsydra.

The laws of circular motion were next investigated by Huygens; and a combination of these laws with Kepler's Third Law, on the assumption that the planetary orbits round the Sun are circles, leads at once to the Law of Attraction varying inversely as the square of the distance.

This law, generalised into the Law of Universal Gravitation, became in Newton's powerful hands the foundation of his system of Natural Philosophy, in explaining not only the elliptic orbits of the planets in accordance with Kepler's first two Laws, but also the perturbations of these orbits as exemplified in the Lunar Theory, and the Theory of the Tides.

Prof. Mach suggests in Fig. 139 a very ingenious experimental illustration of the tides on a body in free space, like the Earth, as distinguished from the tides which would be produced if the Earth was fixed, by means of a small iron sphere covered with a solution of magnetic sulphate of iron; this can either revolve as the bob of a conical pendulum in true planetary style round the pole of a fixed magnet, representing the disturbing Sun or Moon; or it can hang suspended at rest at a small distance from the pole, and thus illustrate high water under the disturbing Sun or Moon, and low water at the antipodes when the Earth is supposed fixed. The discrimination of the two cases must be considered one of the most brilliant parts of the "Principia."

It is curious that Prof. Mach does not accept Newton's distinction between the relativity of motion of translation and the absoluteness of motion of rotation, illustrated experimentally by Newton by means of a revolving bucket of water suspended by a twisted rope ("Principia," Definition VIII. *Scholium*); and here we think he would have been interested in Maxwell's arguments on Rotation in § 104, *Matter and Motion.*

Maxwell proceeds to explain that it is possible, by means of observation and experiment on or inside the Earth alone (by Foucault's pendulum, for instance), to disprove Milton's assumption, that it is evidently all the same

> "Whether the sun, predominant in heaven,
> Rise on the earth, or earth rise on the sun;
> He from the east his flaming road begin,
> Or she from west her silent course advance;"

&c., although the geometrical configuration of the earth and the heavenly bodies, so far as is discoverable by astronomical observation, is the same on either assumption.

In the Translator's Preface we are told that "Mr. C. S. Pierce has rewritten § 8 in the chapter on Units and Measures, where the original was inapplicable in this country (America) and slightly out of date."

As might be anticipated, this means that we are now to change the name of the quantity formerly designated by the word *weight*, *poids*, *gewicht*, *pondus*, and to use the word *mass* instead.

But if a continental mathematician, Prof. Mach included, is asked to give a numerical definition of the mass of a body, he replies, if in French, "poids divise par g"; so that if a body weighs p kilogrammes, its mass is $\frac{p}{g}$, and the unit of mass is thus g kilogrammes.

When the gravitation unit of force was in universal use, it was considered an abbreviation to write m for $\frac{p}{g}$, and to replace the gravitation measures of momentum and energy, $\frac{pv}{g}$ and $\frac{pv^2}{2g}$, by mv and $\frac{1}{2}mv^2$.

But when Gauss's absolute unit of force is employed, the absolute measures of momentum and energy are pv and $\frac{1}{2}pv^2$, and the abbreviation of m for $\frac{p}{g}$ is no longer required; or if the letter m is employed, then $p=m$ and not mg, as Mr. Pierce asserts; if the mass of a body is 10 kilogrammes, its weight in kg cannot be anything except 10 kilogrammes.

But if with absolute measures we retain the equation $p=mg$, and measure m in kilogrammes, then p, the *weight* or *poids*, is measured in one-gth parts of a kilogramme; this is contrary to all practice, and is absolutely forbidden by the laws on *Weights and Measures.*

With gravitation units the *weight* of a body is at once the numerical measure of the quantity of matter in the body (Newton's *quantitas materiæ*) and of its gravitation, or the force with which it tends to the Earth; if a body weighs p kilogrammes, it is attracted by the Earth with a force of p kilogrammes. The loose definition usually given that "the weight of a body is the force with which it is attracted by the Earth," is really no definition at all, but a mere description; it should at least be amended to "the weight of a body is the *number of units of force* with which the body is attracted by the Earth"; and it will be found that this definition is never employed except with the gravitation unit of force; so that this definition merely asserts in a roundabout way that the weight of a body is a measure of the quantity of matter, as measured out by weighing against pound or kilogramme weights.

It is incorrect to say that there are two systems of *measurement*, the *absolute* and the *gravitational* (p. 284). There is no practical method for the measurement of forces in absolute measure with any pretence to accuracy; the absolute system is merely a system for recording numerically the results of experiment; the measurements themselves are always made in gravitation measure, and afterwards converted into absolute measure by multiplying by the local value of g. There is thus no need for absolute units with our insular British F.P.S. (foot-pound-second) system; and Prof. James Thomson's *poundal*, although a convenient name, is of no practical or theoretical use.

In experiments at Washington, the Paris gravitation unit would not be employed, so that the statements on p. 286 do not tend to clear up the subject. What appears to be meant is that if a perfect spring balance could be constructed such that a kilogramme deflected it at Paris through 981 divisions, then when carried to Washington the deflection would fall to 980·1 divisions, and if carried to the Moon to about 164 divisions, but if carried to the surface of the Sun the deflection would rise to about 30,000 divisions, provided Newton's "Law of Universal

Gravitation" is correct; but the kilogramme weight remains the same throughout the universe.

But if a balance could be constructed with its fulcrum somewhere in the Azores, and the scale pans hanging over Paris and Washington, then 1 kilogramme at Paris would equilibrate 1·00092 kilogramme at Washington (p. 286).

It is sometimes asserted in our school-books on Mechanics that a pound weight, if carried to the surface of the Sun, would weigh about 30 pounds; but if a balance could be made to stretch between the surfaces of the Earth and of the Sun, then a pound weight at the Sun's end would be equilibrated by 30 pounds at our end; so that it is equally true to say that a pound weight on the Sun will weigh 30 pounds on the Earth.

Prof. Mach would perform a great service if he would extend his criticisms on the usage of the word *pondus* by Huygens, Leibnitz, Wallis, &c., to modern times, as great mystifications exist at the present day.

The C.G.S. system of units is explained by Mr. Pierce on p. 285. These units are much too minute except for recording delicate physical measurement. If Prof. Johnstone Stoney's amendment of the M.K.S. (metre-kilogramme-second) system had been adopted, we should have a system incorporating the joule, watt, volt, ohm, and ampère as units; it is not too late to make the change; the disadvantage that the density of water is 1000 in this system is more apparent than real.

Although the double system of natural laws mentioned by Rankine is now exploded, we still have a double system of instruction in mechanical text-books, one theoretical, geometrical, rational; the other practical, mechanical, empirical, discoverable by experience. It should be the object of modern science to break down the barriers between these two systems, and to treat the subject of mechanics from one point of view. Instead of this, the gap between the two systems seems to be an increasing one, insomuch that Prof. A. B. W. Kennedy, in his inaugural address to Section G of the British Association at Oxford, demanded for young engineers a course of instruction in mathematics entirely different to that imparted at present in our schools and colleges.

Some remarks at the end of Prof. Klein's Sixth "Evanston Colloquium" may be consulted as bearing on this question.

A careful study of Prof. Mach's work, and a treatment with more experimental illustration, on the lines laid down in the interesting diagrams of his Science of Mechanics, will do much to revivify theoretical Mechanical Science, as developed from the elements by rigorous logical treatment.

A. G. GREENHILL.

NEWTH'S INORGANIC CHEMISTRY.

A Text-book of Inorganic Chemistry. By G. S. Newth, F.I.C., F.C.S. Pp. xiii. 667. (London: Longmans, Green, and Co., 1894.)

THE author states in the preface, that the arrangement of the course of elementary instruction in chemistry, which is given in this book, is based on the periodic classification of the elements. The properties of four elements, *hydrogen*, *oxygen*, *nitrogen*, and *carbon*, and the properties of many compounds of these elements, are considered before the systematic study is entered on, of the groups into which the elements are divided by the application of the periodic law. Then follow chapters wherein the members of the various groups of elements, and the chief compounds of these elements, are described. To this descriptive part of the book are prefixed fifteen chapters of "introductory outlines," constituting "a brief sketch of the fundamental principles and theories upon which the science of modern chemistry is built." In his directions to students using the book, the author says that a start should be made by reading carefully the chapters dealing with chemical change, elements and compounds, nomenclature, and symbols; that the four typical elements—hydrogen, oxygen, nitrogen, and carbon—should then be studied; and that, as this study is proceeding, the remaining chapters of the "introductory outlines" should be mastered.

It seems to me that the method of the author is radically wrong. Descriptive statements of facts ought, surely, neither to precede, nor to follow, but to accompany, the reasoning on these facts whereby general principles are gained.

The author's treatment of chemistry implies that "the science of modern chemistry is built" on the foundation of such generalisations as those stated in his "introductory outlines," and that detailed descriptions of the properties of certain kinds of matter called elements and compounds constitute the science that is raised on this foundation. Would it not be more advisable so to treat the subject as to show that chemistry rests on certain definite natural facts, but that only when these facts are compared, contrasted, and classified, does a scientific knowledge of them begin?

The descriptions in this book of the members of each group of elements seem to me to be exceedingly well done; many portions of the chapters treating of principles and theories, notably the pages which deal with the laws of chemical combination, are admirable; nevertheless the book, as a whole, gives the impression of being unscientific. The method of the book tends, in my opinion, to perpetuate the vicious and unreal distinction between chemistry and chemical philosophy, a distinction that has probably been as potent as any other cause in stopping the progress of the science.

The purely descriptive portions of this work are often extremely good, as far as they go. The facts, or rather half-facts, are stated in a clear and orderly way; care is taken to notice recent work of importance; the woodcuts are well executed: but, all this is only the material out of which chemistry is constructed.

About 130 pages are devoted to statements of the properties of four typical elements—hydrogen, oxygen, nitrogen, and carbon—and of the properties of many compounds of these elements. But hydrogen, oxygen, nitrogen, and carbon are not treated as typical elements they are not compared and contrasted with other elements. After reading the descriptions of these elements and their compounds, and studying the properties of the elements in each of the eight groups of the periodic classification, one still feels unsatisfied. Something is lacking. Surely some fair and fitly fashioned building ought to rise on this broad superstructure. If chemistry is to be treated as a recitation of disconnected, or artifi-